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are more specifically those of self-consciousness have been treated by him with a curtness and an inadequacy wholly unworthy of his own skill and experience. The result is that he almost wholly abandons the only region where, despite our limitations, we finite beings have the least comprehensible indication, the least concrete and reasonably intelligible exemplification, of how one individual system *can* "bring home" and "suppress" and yet "keep" its own particulars. The consequence is — the metaphors aforesaid, and the almost pathetically inadequate conclusion of a magnificently begun and in part most admirably conducted enterprise, where learning, dialectical skill, depth, ingenuity, and delight in the spirit of free inquiry, have conspired to make many of the chapters almost classical in their excellence, while the whole book remains so fragmentary in its impression.

JOSIAH ROYCE.

On the Perception of Small Differences. By G. S. FULLERTON and J. M. CATTELL. Publications of the University of Pennsylvania. Philadelphia, 1892. — pp. 159.

The material for this treatise on the Perception of Small Differences was derived from a large number of experiments on the extent, time, and force of movement with the arm, on lifted weights and on lights. Under most of these divisions experiments were made according to four psychophysical methods, and commonly with several subjects, — the object being less to establish a 'law' than to investigate the mutual relations of the psychophysical methods, the value and meaning of variable and constant errors, the effects of practice, and "the significance of the confidence of the experimentee in relation to objective correctness."

As regards the psychophysical methods, the authors find the method of just observable difference unsatisfactory, the method of right and wrong cases the most accurate, and the method of average error the most convenient of all the methods. Merkel's method of doubled stimuli, and the method of mean gradations, classed together under the heading, "method of estimated amount of differences," give variable results, and the authors conclude that in these methods the "observer probably does not estimate quantitative relations in sensation, but quantitative differences in the stimuli learned by association" (p. 152). The experiments leave no doubt in regard to the faultiness of the method of j. o. d.; the results contradict each other and the results gained from other methods. At the same time, in so far as the method of minimal changes is to be classed under the general heading of j. o. d., the experiments on the extent, form, and time of movement leave the question untouched. From the very nature of the case, where the 'experimentee' himself produces the norm, and then seeks to bring a second sensation or complex of sensations into a given relation with the norm, no method of gradations could be employed, and therefore none of those precautionary measures, which, in the opinion of the writer, make the m. of m. c. as valid as the error methods, were available. Then, too, in experiments of this kind on movement, aside from the great complexity of the perceptions involved, there comes in a disturbing factor which is not generally present in the comparison of sensations of the special senses, *viz.*, between the norm and the compared stimulus there intervenes a third complex of sensations due to the return of the limb to its initial position. If, in getting the j. o. d. between two tones, a third tone very different from either should intervene, it would seriously affect the judgment.

The writer must confess to a feeling of surprise to find the so-called method of doubled stimuli lumped together with the method of mean gradations under the general heading of "method of estimated difference." Beside Merkel, the inventor of this method, no one, so far as I know, has taken the method seriously, — certainly no accepted conclusions have been drawn from its use. As Wundt first suggested, and as Fullerton and Cattell assert, the estimation of one stimulus as double another is a matter of association and experience. A complete analysis of the method of mean gradations would take too much space; it may, however, be pointed out that this method is generally regarded as a valid one, and that its validity has been tested by searching investigations which were independent and mutually corroborative (Neiglick, Lehmann, Ebbinghaus). So far as association and experience play any part in it whatever, they play a disturbing part, and the more such influences are removed, the more accurate and constant the determinations become. The difference between the two methods is a vital difference, — in the method of doubled stimuli we find a direct comparison of sensations, — in the method of mean gradations we find a comparison of differences of sensation.

Of especial worth in this treatise are the conclusions drawn from a comparison of the results of the several psychophysical methods in the same line of investigation, and from acute evaluation of the data.

Thus, in the experiments on the force of motion, by combining methods, the authors were able to show that the just observable difference meant a different thing to different observers, that the variable error of the j. o. d. was a better test of discrimination than the j. o. d. itself, and that the constant error cannot be taken as a measure of discrimination in experiments on the extent of movement by the method of average error. The investigation on extent of movement supplements Delabarre's work on sensations of motion in considering the relation of the variable error to the size of the movement. Indeed, the experimental part of the work abounds in acute observations of value to the student of the psychophysical methods.

For extent and force of movement it was not found that the error of observation increased as fast as Weber's law demands; instead of being proportional to the magnitude of the stimulus, it was more nearly proportional to the square root of the stimulus. Accordingly the authors, or more especially Professor Cattell, propose to substitute for Weber's law the following: — "The error of observation tends to increase as the square root of the magnitude, the increase being subject to variations whose amount and cause must be determined for each special case" (pp. 25 and 153). As an independent law and a generalization from the experiments under the given conditions, the statement is hardly to be criticised, but the conditions are scarcely such as to make it a substitute for Weber's law. The enormous complication of the sensations entering into the perception — sensations which arise in different parts of the limb in different parts of the movement — makes it almost impossible to consider Weber's law as bearing any relation to the perception. For the law expresses primarily the relation of a certain kind of sensation to a given intensity of stimulus. Experiments made by the writer's students indicate that the proportion of the square root holds true not only for movements in the same direction, but for consecutive movements in opposite directions — in which case, whatever the relation at issue, it is certainly not that of a certain sensation to a given amount of stimulus.

The treatise is enriched by descriptions and cuts of several pieces of apparatus devised by the authors — a dynamometer adjustable for force and extent of pull, apparatus for measuring the time (velocity) of movement, and an apparatus for comparing successive light stimuli.

F. ANGELL.